

rocks of much coarser grain than the modern ones. But I am not aware that this is the case. Again, to understand such blasts, the earliest trees should have trunks of enormous thickness and their leaves must have been very tough, or they would have been torn to shreds. There seems to be no reason to suppose that the trees of the Carboniferous period present marked peculiarities in these respects."

"It is on these grounds that I venture to dissent from Mr. Ball in the geological interpretation to be placed on the tidal theory, and I think we must put these violent phenomena in pre-geological periods."

But is it necessarily true that the prodigious tides must have produced a coarser material as the result of their grinding than is found in the later rocks? I can imagine it to be contended that the more powerful mill would produce the finer flour, but in truth I really do not see that we have any *a priori* grounds for deciding whether the *débris* produced by mighty tides should be fine or coarse. Have we not illustrious authority for invoking our "Domestic Productions" to throw light on obscure questions removed from actual observation. Let us look at the biggest tides we know of, and see whether they are associated with fine mud or with coarse. I appeal to every one who has stood on the Clifton Suspension Bridge or walked on the Beach at Weston-super-Mare to answer this question. In both cases they will see mud of a fineness and a stickiness that is proverbial; yet that mud is washed twice every day by the mightiest tides in the British Islands. I do not say, nor do I believe, that the fineness of the mud in the Avon is the consequence of the great tides; but I think the illustration is a fair reply to an argument which says the tides in ancient days cannot have been of great size, because the mud with which those tides are associated is not coarse.

In the second place, Mr. Darwin urges against me the trees of the Carboniferous epoch, and his inference that the tremendous tides cannot have existed in the Carboniferous epoch is probably well founded. But I have not said that these tides did exist in the Carboniferous epoch. I can only again repeat that my argument supposed that the mighty tides may have existed in the times when the very earliest stratified rocks were deposited. In the course of ages, as the moon receded, so the tides gradually dwindled down until in the comparatively modern time indicated by the Carboniferous epoch, they may have been small enough to be connected with the wonderful coal vegetation.

I had, as I was bound to do, most carefully weighed the words in which I addressed you from this place last year. I was aware that the opinion I advanced would meet with opposition. This was a reason why I should consider the subject most carefully before I spoke, but it was not a reason why I should withhold the views at which I had arrived. I have again considered the matter with the results now set forth, and I have seen no reason to depart in the slightest degree from the position which I had previously adopted.

MARS¹

THE similarity which has long been thought to exist between our own globe and the planet Mars would naturally commend itself to careful examination at the hands of such observers as possess instruments adequate to the inquiry. The shadowing of large portions of its surface with patches which easily lend themselves to the supposition of being collections of water, the occasional indistinctness of their outlines, so strongly indicative of

atmospheric obscuration, the clothing of either pole with the semblance of a snowy mantle obedient in its extent to solar action, all this would bespeak of itself a critical investigation. And the challenge has been taken up from an early period, and to an extent which would probably surprise those who are unfamiliar with the subject. Already in 1873 the number of drawings collected by Dr. Terby of Louvain, than whom no man is more intimately conversant with areography, amounted to 1092, and the nine subsequent years, which have included among others the celebrated representations of Green and Schiaparelli, have greatly augmented that imposing number. We should be mistaken, however, if we were to estimate the progress of our knowledge by the multiplication of designs. In this case the ancient saying *πλέον ἤμισυ παντός* would probably express too large a proportion. The increase, if in some respects not to be regretted, brings with it additional elements of uncertainty, if not of error. Many representations might be discarded with positive advantage to the final conclusion: like numerical observations whose unworthiness is detected by their wide deviation from the mean of the rest, the result is all the surer for their exclusion. An unpleasant experience proves that the most careful observer is not always the most successful draughtsman, nor in such matters is zeal any pledge of excellence. Comparison of the results obtained by different astronomers leads to the conclusion that, after due allowance has been made for instrumental and atmospheric differences, all men do not see alike, or interpret in the same way what they see, or transfer the image to paper with equal success. Here it is that photography, though not exempt from defects and hindrances of its own, is now beginning to render invaluable aid. But such an object as the disc of Mars would not lend itself very readily at present to the camera, and the pencil and the brush must do the best they can till some further advance is made to supersede them.

But however improved may be our future representations, and whatever may be the result—on every supposition most interesting—of the keen scrutiny that is in store for the next opposition of the planet, it would undoubtedly be an injudicious course to discard as unworthy of study and comparison the delineations of earlier days. Less valuable, if standing alone, they may attain considerable importance in the elucidation of some otherwise unexplained difficulty; and evidence which, unsupported, might be of little weight, may acquire especial consequence from its collateral bearing on more direct testimony. The comparatively rude and defective sketches of a long-passed era, contained in the publication before us, executed in a spirit of unwearied industry and unimpeachable fidelity, but under the influence of a mistaken impression, form a striking illustration of the previous remarks.

The history of the "Areographische Beiträge" is connected with a very lamentable occurrence in the life of the worthy old Hanoverian observer, Dr. Johann Hieronymus Schröter. He had long been settled in a Government office at Lilienthal, not far from Bremen, where his almost innumerable observations on sun, moon, and planets (with stars he did little) had been carried on with reflectors of various sizes—two by Sir W. Herschel of 4 and 7 feet focal length, others by Schrader, of Kiel, of 7, 11, 15, and 27 (26 English) feet, and a 4-inch object-glass by Dollond, equatorially mounted. His passion for observation would never have allowed so interesting an object as Mars to escape him, and accordingly we find that between the years 1785 and 1803 he had accumulated 217 designs, with a corresponding description marked by all the minute preciseness of detail and inference which characterise his other labours. The work had been promised for publication at Easter, 1812, but had been somehow delayed, when an event occurred on the night of April 20, 1813, in connection with the occu-

¹ "Areographische Beiträge zur genauern Kenntniss und Beurtheilung des Planeten Mars." Von Dr. J. H. Schröter; herausgegeben von H. G. van de Sande Bakhuyzen, Director der Leidener Sternwarte. 8vo, 447 pp., with Atlas. Leiden: E. J. Brill.

pation of Bremen by the French, under the rapacious and unscrupulous Vandamme, the story of which we may allow the sufferer to relate in his own words.

"Through the most barbarous fury, in consequence of an equally barbarous sentence, the whole unoffending soft "vale of lilies" (Lilienthal) was, without previous inquiry, destroyed by fire. Without possibility of succour, they burnt down also the Royal Government offices; I lost the whole of my furniture, and what was most distressing of all, with a considerable damage also to the bookshops of Europe, the sole stock of my collected works and writings laid up in the government buildings. Even my observatory, preserved by Providence from the conflagration, was a few days afterwards broken into, plundered, and through the destruction of the clocks, breaking off of the finders, and robbery of the smaller instruments, scandalously ruined. Having been displaced from my post, my income had been previously by degrees so very much reduced, that I was compelled to forego everything but absolutely necessary expenditure, and to be laid aside in a scientific slumber." Elsewhere he says that even his journals had perished; and at the date of writing the introduction to his "Observations and Remarks upon the Great Comet of 1811" (January 22, 1815), from which the foregoing passage is taken, he complains that his circumstances were still so reduced, that his observatory, for want of time and money, remained for the most part in a state of confusion. So great are even the minor miseries of those "wars and fightings," of which many speak with such apathetic unconcern. It is painful to add to these sad details that this seems to have been Schröter's final effort, for after a twelvemonth of bodily and intellectual decay, he expired August 29, 1816, leaving behind him a worthy memory, to which, till of late years, our own country has done but inadequate justice.

The "Areographische Beiträge" remained in MS. at his death, having escaped the calamitous fire, but so narrowly, that two out of the sixteen copper plates of figures had to be engraved again; no idea seems to have been entertained of publication, but they were safely preserved by the author's family. Their existence having been ascertained many years ago, by the present writer, through the kindness of Dr. Peters of Altona, a negotiation was set on foot for their acquisition by the Royal Astronomical Society; this proved ineffectual; but, in consequence of the attention directed to them, they were allowed to be inspected by Dr. Terby, to whose able and comprehensive analysis of the MS. as published in the *Mémoires* of the Belgian Royal Academy of Sciences in 1873, the present notice is deeply indebted; and astronomers will be glad to learn that they have now been purchased by the University of Leiden for the library of that observatory, and that, after an obscurity protracted through seventy years, they have at last been published in a complete and handsome form, under the able and accurate editorship of the director of that institution.

The work, though characterised, like other productions of the same author, by a needless amount of prolixity, is well deserving of careful study, as indicating or confirming some valuable conclusions, and affording material for suggestive thought. The whole observations are pervaded by an impression that the obscurer portions of the disc are condensations in a vaporous atmosphere. The author, with a singular misconception of terrestrial analogy, supposes throughout that such cloudy masses viewed on their upper or enlightened side would appear darker rather than lighter than the surface beneath them; admitting at the same time that the configuration of that surface may so modify the superjacent atmosphere, as to cause a permanence, or, at any rate, recurrence of vaporous formation, from which the rotation may be, and has been determined. The occasional invisibility of dark spots, which has been recorded by too many observers to

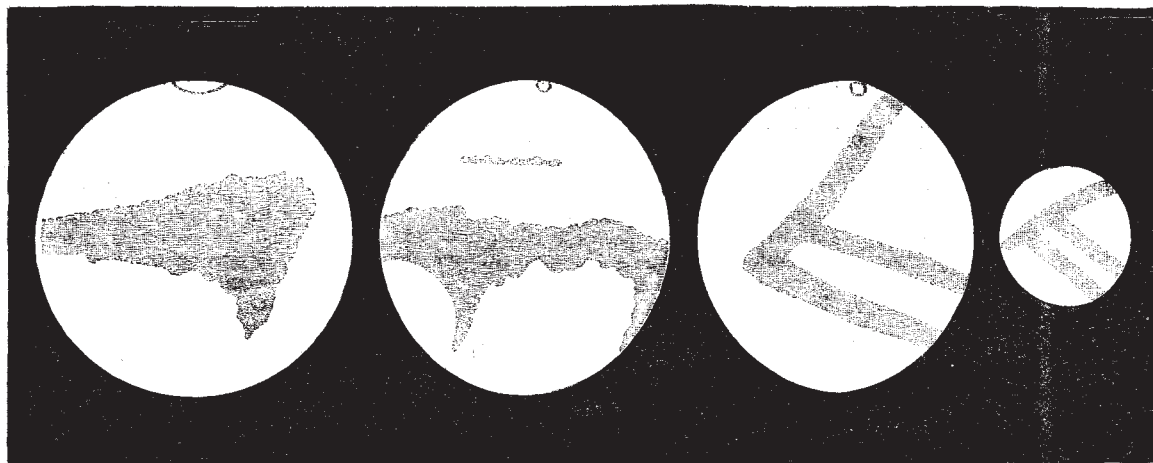
be brought in question, would be explained by Schröter in accordance with this theory, and may possibly be due to atmospheric causes; though, as Terby has pointed out, it may often have arisen from the difficulty of tracing any markings in the neighbourhood of the limb. It is more difficult to account satisfactorily for the movements ascribed by Schröter to the action of winds, of which he has specified in an elaborate table no less than forty-six instances, not much differing in velocity from those on earth, and in the great majority of cases conspiring with the direction of the rotation. Error may have crept in with regard to the identification of some of the spots; and both from the designs and descriptions a suspicion arises that some of the minuter details, which now serve for the recognition of distinct but similar regions, escaped the observer's notice. There must have been some cause for this unfortunate defect which detracts materially from the value of his work, and the removal of which would have been the one step in advance which, as Terby remarks, would have put him in possession of the true interpretation of what he saw. It would be a ready explanation to refer it to the imperfect defining power of his instruments; and I have somewhere read of a comparison instituted after his time between his much-prized speculum of $9\frac{1}{2}$ inches aperture and $12\frac{3}{4}$ feet focal length, and a Fraunhofer object-glass of, I think, about 4 inches, to the disadvantage of the former. Yet on the other hand he began his work with telescopes by the elder Herschel; and his 7-foot instrument by this great maker does not seem to have been superior to that of the same dimensions by Schrader, the manufacturer of all his larger ones. This point is therefore not quite clear. We might have attached some importance to his own admission, in his work on the moon, that his vision was less microscopic than that of his assistant, Harding, but for the fact that the latter occasionally aided him in these observations on Mars. Whatever may have been the cause, the frequent absence of minuter detail must have served to confirm Schröter's misapprehension of what he saw. And it must be borne in mind, in estimating his observations in general, that it was his habit to undertake investigation with a preconceived idea. In the case of the moon, his prepossession in favour of changes no doubt occasionally misled his judgment; and on Mars he might be prepared to look out for atmospheric movement by the known phenomena of Jupiter. An anticipation of this kind may not be without incidental advantage in directing and sharpening attention; our search may be aided, and with perfect fairness and honesty, by the foresight of the result: but at other times such expectations may be equally or more prejudicial; and they probably were so in the instance before us. And it is at any rate a possible suggestion that, with regard to some of these supposed changes, Schröter's ideas may have been unconsciously biased by his study of the surface of Jupiter. The perspective foreshortening of that great globe having no effect on the aspect of its more conspicuous and familiar markings, from their equatorial direction, and being non-apparent in the transits of the shadows of the satellites, the eye may come to regard it too much as a flat disc, and to appreciate too little the extensive changes which mere foreshortening produces among markings arranged in any other direction.

But whatever explanation may be attempted of Schröter's illusion, which, as his editor remarks, increases the value of his figures by securing their perfect independence, or however we may regret an apparent want in some cases of more distinctive detail, there can be no doubt that the work is worthy of attentive study. Dr. Terby has pointed out one curious inference—that at that epoch certain dark markings bore a relative proportion to each other, too different from that which obtains at present to be easily explained away. Nor does this result by any means stand alone: and it has considerable value as affording

collateral testimony to subsequent observations pointing in the same direction. To say nothing of other authorities, the accurate designs of the deeply-regretted Burton, and the latest delineations of Schiaparelli (independent of the wonderful duplication of the narrow streaks) concur with the drawings of Schröter in indicating one of two suppositions as regards the dark patches of Mars; either they must be liable to long-persistent and very deceptive alteration of visible outline from atmospheric causes, or their own extent must be so variable as to awaken a doubt whether the right key of the mystery is after all in our hands. We have long believed that we hold it, and terrestrial analogy has been thought sufficient to account for all we can see. The result of the next opposition in 1884 may be found to confirm the old hypothesis; but it is not beyond possibility that it may shake it, even past recovery.

Besides the conclusion thus briefly indicated, several other points of varying degrees of interest are touched upon in this comparatively bulky treatise, some of which we may refer to, though only with a passing notice. Schröter paid considerable attention to the polar whiteness; but while he admits the probable analogy of terrestrial snow, he is less confident than some other ob-

servers as to any marked influence of solar radiation. Terby, however, has pointed out the cause of his misapprehension, and his substantial agreement with his compeers. He was aware of the irregular outline of the snowy regions, and thought them slightly different in colour, the south pole verging towards yellow, the north blue. On the question of rotation he could obtain no satisfactory result, as might have been expected from his idea as to the instability of the markings; his values being discordant at different periods: the mean, 24h. 39m. 50.2s., was only about 29s. less than that of Sir W. Herschel, but very wide of Proctor's elaborately deduced value, 24h. 37m. 22.735s.—a fact pointing probably to the same conclusion as before, that from some as yet imperfectly explained cause, the exact position in longitude of some of the features of the planet is not fully ascertained. The amount of the polar flattening of Mars is, as is well known, matter of much uncertainty. Herschel made it as much 1-16; Dawes, nothing, or even negative. Our observer, nearest to the great English authority, found it less than 1-81, a quantity fairly evanescent. The method of measurement which he adopted throughout all his researches was that of the apparatus which he calls the "projection machine." In this simple



contrivance both eyes are employed simultaneously, the one in viewing the telescopic image, the other in bringing to coincidence with it, a squared-out area in the case of the moon, a series of discs for the planets, in either instance with provision for varying distance and illumination. This binocular mode of measurement, if open to some sources of error not incidental to the ordinary apparatus, appears hardly deserving of the censure so freely bestowed upon it by Beer and Mädler, who were not always fair towards the labours of Schröter; and notwithstanding the perfection to which the wire micrometer has been brought, might perhaps be revived for some purposes with advantage. The diameter of Mars obtained in this way by Schröter, 9".84, does not differ much from the 9".8" (a curious instance, by the way, of notation by thirds), of Sir W. Herschel, or from more modern values—some proof, it may be thought, of the competency of the apparatus to obtain a close approximation.

Observations included in this volume of a partial flattening of the limb of Mars and of the abnormal breadth and want of symmetry in the phasis, however improbable they may at first appear, are not without parallel in the case of other planets, or the experience of other observers. If, as it must be assumed, these are nothing more than illusions, the record of them is still

valuable in the probable event of their occasional recurrence.

To this brief and imperfect notice are appended three sketches from the Atlas—the two first as specimens of Schröter's mode of delineation—the third as bearing so striking a resemblance to one of my own, shown on a smaller scale beside it, that it might, in the absence of more accurate data, serve as the basis of an approximate value of rotation.

The dates respectively corresponding to these designs are 1798, Sept. 9d. 8h. 4m.; Oct. 17d. 7h. 39m.; Nov. 13d. 7h. 10m.; 1862, Dec. 10d. 9d. 30m.

T. W. WEBB

DESTRUCTION OF LIFE IN INDIA BY POISONOUS SNAKES

IN January, 1870, being then in Calcutta, I collected statistical information which afforded proof that the loss of human as well as animal life in India from the bite of venomous snakes was very great; and as it seemed to me that this ought to be, to a great extent, preventible, I extended my investigations with the view of obtaining accurate information as to the characters and peculiarities of the venomous snakes themselves, the localities in which they most abound; the *modus operandi*